

Interactive comment on “Changes of the Arctic marginal ice zone” by Rebecca J. Rolph et al.

Anonymous Referee #1

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This paper shows that there is no trend in the areal extent of the marginal ice zone (MIZ), an increase in the fractional area that the MIZ covers in the total sea ice extent, and that the CICE-CPOM model fails to reproduce these observations.

I think the observation that the total areal extent of the MIZ hasn't changed is an interesting way to reconsider the dramatic changes in Arctic sea ice, but this isn't really a new insight. For example, Strong and Rigor (2013) and other studies have shown that the MIZ moved northward and its width has increased. Rolph et al. is simply arguing that the glass is half full (no change in MIZ extent), rather than half empty (MIZ width is increasing). While I think this is an interesting way to look at the changes in Arctic sea ice, does this different perspective provide any new scientific advances? The authors also need to consider that the sea ice concentration data has larger errors during summer than they assume. As this paper currently stands, I don't think it provides enough compelling reason to warrant publication.

C1

Major Comments:

1) Why is it important to consider that the areal extent of the MIZ hasn't changed? The authors need to beef up their case that it is important to think of the changes in the MIZ this way. Can the authors show how this perspective provides new insights that the many physical process studies of changes in the fractional area of young ice versus old ice do not? Or new insight into some biological process?

2) The errors in the sea ice concentration retrievals from passive microwave satellites during summer are large. For example, in their figure 3 they show wildly varying estimates of where the northern edge of the MIZ is. Some (Walt Meier and/or others at NSIDC or NASA may have a paper on this) have estimated the summer SIC error to be higher than 40% during summer, and most of this error and differences between the retrieval methods is related to how they filter weather. Rolph et al. need to provide a more thorough error analysis than assuming an overall 10% error estimate since the errors in the SIC retrievals affect how robust their conclusions are.

3) The fact that models don't reproduce these observations isn't surprising. There are already many papers that show that various models don't reproduce some observation. But as with any tool, does simply showing that a tool doesn't work for this job warrant publication? If Rolph et al. could pin down what needs to be improved in the models, that would advance science and the inclusion of the model study would be interesting.

Minor Suggestions:

4) Be consistent in your use of units. E.g. in lines 194-195 you switch between meters squared to kilometers squared. I suggest sticking with kilometers squared.

5) Need to note 10^7 in the label for the Y axes in Fig. 1 rather than “1e7” on the top corner of the plots.

6) Provide a short section 3.3 discussing how statistical significance was estimated. Maybe just move this from caption of table 1.

C2

7) Caption of Fig. 1: Change "...is defined at..." to "...is defined as...".

Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2019-224>, 2019.